We have not, of course, made use of this new volume practically; but from what we see of it, we should, without hesitation, recommend it to any person beginning the delightful study of mosses as the most likely of all those within our knowledge to suit his needs. E. F.

OUR BOOK SHELF.

Catalogue of the Described Diptera from South Asia. By F. M. Van der Wulp. 8vo. Pp. 220. Published by the Dutch Entomological Society. (The Hague: M. Nijhoff, 1896.)

COMPARATIVELY few entomologists interest themselves in *Diptera*, and therefore the number of species of the order enumerated in the present catalogue is only 2889, and doubtless represents only a small percentage of those actually existing in the rich fauna which it samples; for the *Diptera* are probably the third most numerous order of insects, surpassed only, according to the indications of our present knowledge, by the *Hymenoptera* and *Coleoptera* in the total number of species which they may be expected to include. Prof. Van der Wulp is recognised as one of our first living authorities on *Diptera*, and his work will prove of great use to specialists, especially as M. Bigot's "Catalogue of the *Diptera* of the Oriental Region," published in the *Journal* of the Asiatic Society of Bengal for 1891 and 1892, is both imperfect and inaccurate. The introductory part of the work is written in English, and includes a "Review of the Literature of Oriental Dipterology" and a bibliographical list of books and papers consulted. There is also a table of contents at the beginning, and an index of families and genera at the end. We cannot have too many books of this description; for although the number of undescribed species of insects is enormous, it is perhaps even more important to attempt to keep pace with the rapidly-accumulating mass of descriptive matter by means of carefully compiled monographs and synonymic reference catalogues, than to confine our energies to piling up additional descriptions by the hundred or the thousand.

W. F. K.

History of Modern Mathematics. By David E. Smith. (London: Chapman and Hall, Ltd., 1896.)

"HIGHER Mathematics," edited by Mansfield Merriman and Robert S. Woodward, is a text-book for classical and engineering colleges, and is a work containing 600 pages. Each chapter is written by a different author, and is devoted to some special branch of mathematics; chapters i., ii., iii., &c., dealing with solutions of equations, determinants, and projective geometry respectively. The eleventh and last chapter, a reprint of which we have before us, is written by Mr. David E. Smith, of the Michigan State Normal School, and deals with the "history of modern mathematics." Of course it has not been intended here to give a complete history of modern work, but just a sufficient survey of the whole domain to give a student an intelligent idea of the way in which the more recent advances have been made, and the ends gained thereby. Each mathematician has, as a rule, his own speciality; but each of these is one link in the chain which, when put together, forms the whole. Such a history as Mr. Smith gives here fulfils this point, and its shortness and conciseness will be favourable to students of mathematics. The text is increased in value by the numerous footnotes, and a short bibliography is given at the end; this latter is, however, by no means complete, as the author remarks, but he gives references for those who wish to go further afield. For a biographical table of mathematicians he refers to Fink's "Geschichte der Mathematik," p. 240, and for the names and positions of living mathematicians to the "Jahrbuch der Gelehrten Welt," published at Strassburg.

Graphical Calculus. By Arthur H. Barker. (London: Longmans, Green, and Co., 1896.)

A VERY timely book; and useful to instructors in the elements of the subject in providing a number of apt and eloquent illustrations of fundamental ideas. It represents a series of lectures addressed to engineering students, liable to be repelled by pure abstractions, and preferring concrete representations in which their ideas can take root; a complete contrast to the ordinary mathematical text-book of the school of Todhunter. The author should point out that the gradient of 1 in 100 (p. 13) means an angle whose tangent is 0·01 only in the indoor mode of reckoning on a plane; but that in construction of the railway, the angle is made with a sine of 0·01; the two modes of measurement are indistinguishable practically.

Integration is introduced simultaneously with differentiation, as in many respects a simpler idea to grasp; we can realise the growth of a tree at the end of a year, although the rate of growth is imperceptible. Our ordinary mathematical text-books make the mistake of keeping integration in the background too long. G.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

The Utility of Specific Characters.

I MUST confess to still feeling some difficulty in understanding my friend Prof. Lankester's position, notwithstanding his explanations.

The correlation principle was arrived at by Mr. Darwin after a careful examination of a large body of facts. I quote the carefully considered words in which he sums up his conclusions:—

"Correlation is an important subject; for with species, and in a lesser degree with domestic races, we continually find that certain parts have been greatly modified to serve some useful purpose; but we almost invariably find that other parts have likewise been more or less modified, without our being able to discover any advantage in the change. No doubt great caution is necessary in coming to this conclusion, for it is difficult to overrate our ignorance on the use of various parts of the organisation; but, from what we have now seen, we may believe that many modifications are of no direct service, having arisen in correlation with other and useful changes." ("Animals and Plants under Domestication," vol. ii. pp. 354-5.)

It does not appear to me that there is anything in this which conflicts with the doctrine of the "utility of specific characters." The non-useful parts of the correlated chain (if any) are sustained by the useful, and the *whole* seems to me part of the "specific character." If Prof. Lankester had no other object but to call attention to Mr. Darwin's correlation principle, I think this was a little superfluous, for it is part of the mere grammar of Darwinism.

But the point of his speech at the Linnean Society, and of the subsequent account he gave of it in NATURE, appeared to me to go a good deal beyond this, and to be of considerable interest and importance.

In the cases cited by Mr. Darwin, the correlated structures are almost all, to use Prof. Lankester's words, "obvious and measurable." This we would expect in the correlated variation of homologous parts on which Mr. Darwin lays such stress, and which form the bulk of the instances which he gives.

Prof. Lankester's "suggestion" was that "obvious species marks may be only superficial and non-significant phenomena correlated . . . with other less obvious but really important life-saving peculiarities, which might well escape the observation of the describer of specific characters." He then adduces Wells's theory as "a case which seemed to [him] most striking and suggestive in connection with the utility of specific characters." And so I think it is. I ventured to express an opinion that if established it would prove very damaging to, at any rate, the universatility of that doctrine. I certainly supposed that that was Prof. Lankester's object in bringing it forward.

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He now adds that he might as well "have used any of the other cases collected by Mr. Darwin." It is not a very material point, but I do not find that Mr. Darwin makes any reference to Wells's theory in his discussion of correlation, nor do I see any in the body of the sixth edition of the "Origin of Species," though a passage is quoted from Wells's paper at p. xi. of the "Historical Sketch" which is prefixed to it. It had, however, independently occurred to Mr. Darwin, and he discusses it in a somewhat different connection in the "Descent of Man" (i. pp. 242–245). He remarks:—"That the immunity of the negro is in any degree correlated with the colour of his skin is a mere conjecture; it may be correlated with some difference in his blood, nervous system, or other tissues." And he concludes:—"I endeavoured with but little success to ascertain how far it held good." Elsewhere he gives cases to show that "differences in colour are correlated with constitutional differences." But these, though interesting, seem to me too obscure to found any definite conclusion upon. And no attempt is made to show on what material basis, subject to variation, the constitutional difference depends.

The correlation principle as originally defined dealt then with obvious and measurable characters. It is extended by Prof. Lankester's "suggestion" to what is obscure, may be unknown, and perhaps unknowable. In considering the probable utility of any specific character we shall, if the extended principle be accepted, be always open to the objection that we cannot show that the character is not the outward and visible sign of some unobservable internal peculiarity. But that is a position which I do not think we are bound to accept till something more than a hypothetical case has been established.

To sum up: Mr. Darwin based the correlation principle on what is concrete and tangible; Prof. Lankester extends it to what is intangible and hypothetical. It is not a question of what is "apostolic and orthodox," but of what is susceptible of reasonable proof.

As I do not propose to continue this discussion any further, I will take the opportunity of saying that I think it is a matter for regret that, as Prof. Lankester was present at the meeting of the Royal Society when Prof. Weldon's paper was read, he did not deliver himself on that occasion of his somewhat belated criticism. Prof. Weldon's work is of extraordinary interest, and one cannot but admire the self-sacrifice with which such laborious investigations have been prosecuted. If they want a defence, I think the following passage from the "Origin of Species" supplies it.

"It may metaphorically be said that natural selection is daily and hourly scrutinising, throughout the world, the slightest variations; rejecting those that are bad, preserving and adding up all that are good; silently and insensibly working, *whenever and wherever opportunity offers*, at the improvement of each organic being in relation to its organic and inorganic conditions of life. We see nothing of these slow changes in progress, until the hand of time has marked the lapse of ages, and then so imperfect is our view into long-past geological ages, that we see only that the forms of life are now different from what they formerly were." (Sixth edition, pp. 65–66.)

I do not myself see how the slow and ordinarily imperceptible, but inevitable action of natural selection can be demonstrated except by the statistical method. But, firmly as I believe in the inevitableness of that action, I confess that the results attained by Prof. Weldon surpassed my expectations. I am unable to agree with Prof. Lankester, that the investigation does not satisfy the canons of scientific inquiry. The hypothesis on which it appears to me to be based is, that the mean configuration of any organism at any moment is an optimum. In order to test that by the statistical method, the choice of measurements is a mere matter of convenience.

W. T. THISELTON-DYER.

Kew, August 29.

Thermometer Readings during the Eclipse.

I STARTED on July 30 in the *King Harold*, and arrived at Vadsö on August 6. On board this vessel, amongst others, were Prof. Rambaut and Dr. Hugh R. Mill, of the Geographical Society, who I see has sent a note which appears in NATURE of August 27, as to some observations of temperature he took during the eclipse. I was constantly with Prof. Rambaut on the island at Vadsö, and he particularly requested me to observe the temperatures of sun, and shade thermometers during the eclipse at the position he had taken for his observations, which were specially directed to the degree of polarisation of different parts of the corona. I enclose a diagram of my observations, which Prof. Rambaut has suggested I should send to NATURE, should you think they are worth recording. The fall of the sun thermometer (which unfortunately was fully shaded by cloud) was, from 4h. 10m. to just after totality, 2°, and its recovery from that point to 5h. 56m., last contact, was 3°·6. The shade thermometer showed greater variations, viz. a fall of 3°·35, and subsequent rise at 5h. 50m. of 5°·6.

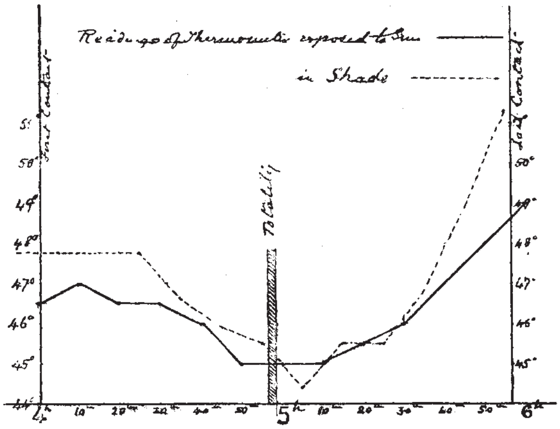

Diagram of observations of sun, and shade thermometers during the eclipse of the sun, August 9, taken at Vadsö.

H. WOLLASTON BLAKE.

8 Devonshire Place, W., September 3.

Sailing Flight.

MR. PEAL (NATURE, vol. liv. p. 317) having again brought up this matter for discussion in the columns of NATURE, I would like to make a new suggestion concerning it, which I have long had on my mind. It will be remembered that Lord Rayleigh (NATURE, vol. xxvii. p. 534) assumed an increase of wind-velocity with altitude to explain the facts of circular soaring, and that quite recently Langley (*Amer. Journ. Sci.*, vol. xlvii. p. 41) has tried to explain the same phenomenon by the assumption—supported in his case by direct observation—that the velocity and direction of the wind is subject to great and rapid changes. Concerning this latter statement, I must say that although in a thunderstorm great irregularities can be observed in the upper air-currents, the shape and relative constancy of small clouds in fine weather seem to show that under ordinary conditions the upper air-currents are much steadier than Langley assumes, and that, therefore, soaring birds can by no means always depend on the presence of wind-irregularities sufficiently great to sustain them. Although no doubt wind-velocities generally increase with altitude, I do not believe that such an increase will *always* be present, nor that it will, when present, be usually sufficiently great to produce the force necessary for raising a bird. We observe, however, that birds do soar nearly always, perhaps even more frequently in fine weather, when the currents are more steady, than in rough weather, when they are more irregular.

Under these circumstances it seems to me that neither Lord Rayleigh's nor Langley's assumptions concerning the source from whence these birds derive the power of overcoming gravity can be correct. It seems to me, doubtless, that a steady horizontal wind of equal velocity in different altitudes *does* enable them to soar and to rise. It is remarkable that this soaring without loss of elevation is always accompanied by *circling*. Elevation is not known to occur without circling, as it might if Langley's views were correct. Were the bird attached to the earth by a string like a kite, it could be and, if the wing-planes were placed in proper positions, would be sustained and raised by a purely horizontal and steady wind. Now it seems to me that the *circling replaces the string*. A circling top retains its position on account of the force in its rapidly circling parts. Could not the soaring bird produce—through circling—a similar stability which, acting like a kite-string, would enable it to oppose itself to the wind, and thus convert the horizontal wind-force partly into a vertical, lifting force? Mr. Peal, in his last letter (*l.c.*) very correctly remarks that the connecting-

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